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REVIEW ARTICLE

Where We Are in Accounting: A Review of "Statement on Accounting Theory and Theory Acceptance"

The first mistake in public business is going into it.
Benjamin Franklin

Nils H. Hakansson

BEGINNING with the 1930's, the American Accounting Association has endeavored to publish at least one comprehensive statement in the area of accounting theory each decade. Logistically, this enterprise has been carried out by a series of committees drawn from the Association's more prominent members.

The first four statements (1936, 1941, 1948, 1957) appear to have come quite close to general expectations in terms of both scope and specific issues addressed. Judged against this precedent, the 1966 statement was rather ambitious and reformist in character, but it still managed, despite a certain change in direction, to retain the basic thrust of its forerunners. The most recent document, *Statement on Accounting Theory and Theory Acceptance* (1977), however, reflects obvious disenchantment with the the basic goals of its predecessors as well as its own committee charge. The result is a sharp turn to the left. The patient, thinking she had a foot problem, ended up with abdominal surgery. The surgeons' report, in essence, attempts to convince us that the surgery was necessary. The section containing the procedural description makes little effort to suppress the spilling of blood and reveals

several infractions. While at times pompous in style, this part is also occasionally incisive. The findings section of the report, however, is simplistic and disappointing. I thought we all knew that even the feet derive most of their nutrition from processing activities centered in the digestive tract.

A SYNOPSIS OF THE STATEMENT

It may be useful to begin my elaboration of the above summary judgment by disclosing the contents of the 1977 Statement in synopsis form:

Chapter 1 quickly sets the tone by stating that "... this report does not attempt to develop a statement of universally accepted theory; instead ours is a statement *about* accounting theory and and theory acceptance. ... Theory acceptance would not be facilitated by this committee's attempting to impose theory closure. ... There is currently an abundance of theories of external reporting" (p. 1), which differ in the way they "... view users and the preparer-user

The author benefitted from helpful discussions with Professor Maurice Moonitz.

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environments. . . . What we seek is a theory that is general enough to cope with this variety and specific enough to offer assistance to accounting policy makers" (p. 3).

Chapter 2 classifies "the dominant approaches that have evolved" into three categories. The first of these is the "classical ('true income' and inductive)" approach, which has been used both by "normative deductivists" (Paton, Canning, Sweeney, MacNeal, Alexander, Edwards and Bell, Moonitz, and Sprouse) and by "chiefly positive, inductive writers" (Hatfield, Gilman, Littleton, and Ijiri) (pp. 5–10, 27–29). The second approach is identified as based on "decision-usefulness" and is also divided into two sub-categories: those which stress decision *models* (Chambers, the AAA 1966 statement, Sterling, *etc.*) and those which focus on decision *makers* (behavioral accounting and aggregate market-level research studies) (pp. 10–21). The third approach is labeled "information economics," and a distinction is made between the "single-individual case" and the "multi-individual case," with considerably greater emphasis given to the latter (pp. 21–25).

Chapter 3 goes on to examine the reasons "... why no theoretical approach has yet achieved dominant acceptance within the accounting community" (p. 31). The obstacles to acceptance of current accounting theories in general are identified and discussed under six headings: (1) the problem of relating theories to practice, (2) the allocation problem, (3) the difficulty with normative standards, (4) difficulties in interpreting security price-behavior research, (5) the problem of cost-benefit considerations in accounting theories, and (6) limitations of data expansion.

With this background, Chapter 4 provides an alternative attempt to "... de-

velop a plausible explanation for the lack of progress in achieving accounting theory consensus" and suggests, by a detailed reference to Kuhn [1970], that what we have is "... the existence of several competing paradigms," defined on pp. 41–42 as "frameworks which 'provide models from which spring particular coherent traditions of scientific research'." It concludes that "... defenders of a particular paradigm are forced to rely on persuasion rather than logic or empirics in attempting to defend a proposal" (p. 48).

The final chapter summarizes the statement in the form of four "messages," which I reproduce *verbatim*:

1. Theory closure cannot be dictated.
2. External reporting theory has a wider scope than that which has been generally perceived.
3. All theory approaches are flawed when viewed from the perspective of some alternative approach.
4. Until consensus paradigm acceptance occurs, the utility of accounting theories in aiding policy decisions is partial.

SCHOLARSHIP AND MARKET INTERVENTION

If we think of the members of the American Accounting Association as primarily concerned with teaching and research, one may legitimately wonder why the AAA has felt the need to publish five statements and 13 supplements on the subject of (financial) accounting principles, standards, and theory since 1936. During this same period, the Committee on Accounting Procedure, the Accounting Principles Board, and the Financial Accounting Standards Board, in turn, have formally represented the much larger practicing arm of the profession in its self-regulatory determina-

tion of standards governing accounting practice. It is clearly not that membership in academia and/or the AAA has precluded one from an active role in accounting rule making; professors of accounting have regularly been called on to serve in this capacity. A more likely reason is that the AAA, through its collective membership, is, from the inside at least, viewed as being in a unique position to give advice on “practical” accounting theory, whether that advice is solicited or not.¹ The FASB, say, may be thought of as having a harder time ignoring a statement published under the auspices of the AAA than any given collection of individual contributions.² In any case, it is worth noting that a similar situation does not exist in other disciplines. The American Finance Association, the American Economic Association, the American Marketing Association, and the American Psychological Association, to name a few, apparently do not feel the need to appoint committees to improve on, or add to, what individual scholars have to say about the state of knowledge in their respective fields. Recall that financial analysts, psychiatrists, and practicing psychologists are also, like CPAs, licensed practitioners.

As eloquently expressed by Chambers [1972] and seconded in the Statement (p. 49), appointed committees cannot conduct research. The very notion contains an antithetical element. Nobel Prizes will continue to be won by individuals and self-selected teams.

The remaining question, then, is whether the 1977 Committee succeeded in its “. . . role to survey the accounting theory literature” (p. 49), *i.e.*, whether it did something that individuals and self-selected teams have not done or would not do—and is, in fact, needed. In other words, to demonstrate convincingly that

a venture such as this one has been successful, it is necessary to establish (1) “market failure”³ in the arena of accounting ideas, excluding efforts by appointed AAA *committees* (but not AAA-funded individual research studies), and (2) that there has been a successful repair of this “failure” by the appointed committee. More specifically, if the study were needed, could not the members voluntarily have formed a team, obtaining the requisite travel expenses from their respective departments or some other agency? Thus, in a sense which is consistent with the discussion on pp. 22–25 of the Statement, a necessary, but not sufficient, condition for this review to be laudable is that the free market in accounting ideas (excluding the AAA’s publishing activities *via appointed committees*), with all its pecuniary and non-pecuniary incentives, has left us with a gap that a survey such as this, conducted by a solicited group of participants drawn from that same market, could fill (by having the committee members engage in limited interaction over a short period of time at AAA expense). That’s a

¹ Recall that the pronouncements of the Committee on Accounting Procedure and of the Accounting Principles Board, before 1965, were not binding on the members of the American Institute of Certified Public Accountants; instead, their authority rested on their “general acceptability.” Consequently, there existed, prior to the mid-sixties at least, a natural temptation for the American Accounting Association to step forward. In this context, it should also be noted that the 1966 and 1977 statements were prepared by *ad hoc* committees, while the 1936 and 1941 statements were issued by the Executive Committee and the 1948 and 1957 statements were developed by a standing committee.

² It may be noted that a proposal to have the American Accounting Association take positions, as a body, on issues before the FASB was recently voted down by the membership.

³ “Market failure” is said to occur when a free market results in a situation which can be improved upon to everyone’s advantage by deployment of non-market forces (market intervention)—one of the central results in economic theory is that this is never possible when the pre-conditions of a competitive market are met.

tall order, apparently taller than anything such a committee could reasonably hope to satisfy.

SCHOLARSHIP AND ACCOUNTING RULE-MAKING

Naturally any self-regulating profession looks to the accumulated body of knowledge relevant to its domain in setting standards and rules of practice. Accounting is no different. In this rule-making task, the applicable rule-making body needs two kinds of inputs: information about how the world works, especially that part of the world with which the profession rubs elbows, and information about what conclusions or implications follow from different sets of premises or assumptions. The first type is descriptive or positive knowledge, pure and simple, while the second may or may not be normative in character.⁴ Only in the last 10 years has a serious *systematic* effort been under way to add to our (scant) stock of empirical knowledge. Most (though by no means all) of the earlier work done by accounting academics was normative in spirit although it often proceeded from unvalidated premises and rarely passed contemporary standards in other fields in coherence and reasoning, with the inevitable result that everyone has been left rather hanging, having gained little in incremental information.

There is still considerable confusion about the distinction between descriptive and normative knowledge in the accounting literature, the 1977 Statement included. For example, the Statement, on p. 33, reads: "... some theories of external reporting incorporate normatively *posited standards* that provide a basis for choosing information to be included in external reports" (emphasis added). The point is that *all the inputs (assumptions) to a normative (accounting) model are posited (i.e. the resources, the technologi-*

cal opportunities, the action space, the probability beliefs, and the preferences). It's impossible for them *not to be*. But they are *all* also subject to verification (via empirical research). Thus, the relationship between "standards" as a surrogate for preferences and utility functions as representations of preferences, for example, must be addressed by analysis. But as assumptions, both representations are empirically testable, at least in principle.

A second example occurs on p. 35, where the question is posed whether security price behavior studies should be interpreted "... as attempts to assess *consequences* or *desirability* of measurement method changes." This is an extreme case of confusion. An empirical study *per se* can only yield *descriptive* knowledge. Desirability, in the public policy sense, is a strictly logical proposition and can only be evaluated in a normative model with a full set of assumptions, including a posited criterion of "goodness"; different models typically give different rankings of accounting alternatives, but the "fit to reality" of each model's assumptions is again subject to empirical measurement.⁵

⁴ The methods employed in descriptive research include observation, examination of documents and artifacts, experimentation, the use of interviews and questionnaires, statistical analysis, and reasoning (both deductive and inductive). In contrast, there is but a single method available for the conduct of normative research—reasoning.

⁵ A few additional slips may also be noted:

- a. The word "normative" is either redundant or inappropriately used in a number of phrases: see *e.g.*, "normative qualities" (p. 13), "normative standards" (pp. 15, 16, 33, 34), and "normative criterion" (pp. 23, 25, 32).
- b. On p. 48, there is a sentence which reads: "Each paradigm implicitly incorporates individual beliefs and premises that cannot be proved or disproved in a logical sense." Premises themselves can never be proved or disproved, although *conclusions* that are presumed to follow from premises can. On the other hand, premises (in an accounting model, say) are *empirically* verifiable, at least in principle.

All decision makers (members of the FASB included) implicitly or explicitly employ normative decision models in making choices of a non-routine nature. The models used may be quite informal and erratically applied, but they do, nevertheless, have the properties of normative structures. These models are only altered or discarded by their users when new and (in their view, of course) better (descriptive or normative) information comes along. In capsule form, this is how accounting scholarship contributes to accounting practice. In particular, research that adds little or nothing will be ignored. As the 1977 Statement suggests (p. 9), the Paton-Littleton [1940] monograph is probably the single most influential work in the accounting literature—on theory as well as rule-making (there is a remarkably close correspondence between the “conclusions” in the monograph and the APB Opinions, for example). But more recent writing most certainly appear to have had a negligible impact, with the possible exception of statistical sampling techniques in the area of audit planning and work in replacement cost accounting on the SEC disclosure requirements in that area. This is, at least, indirect evidence that, despite a high level of activity, the normative research of the last 35 years and our recently initiated formalization of empirical research have not yet led to any *major* advances in accounting knowledge.

To give the preceding assertion more precision, it may be useful to compare the situation in accounting to what has happened recently in our closest sister discipline, finance. There, no less than three recent *academic* contributions have profoundly affected financial practice in the last few years. The first of these concerns the measurement of portfolio risk, where techniques distinguishing between market-wide and firm-specific

risk developed by academics in finance have left no significant area of investment management untouched. The resulting upheaval has been dubbed the *beta* revolution and as such may be characterized as swift and bloodless, although it certainly caused a great many bruised egos.

The second example is in the area of options. Trading in options has increased dramatically in the last few years with the opening of organized markets. While option markets have been likened to gambling casinos, they do, in fact, serve an important economic function as a part of our financial market system [Hakansson, 1978a]. Being relatively risky in isolation but also capable of reducing the investor's overall risk exposure when combined with other securities, options present a challenging valuation problem to investor and academic alike. A large number of market participants, including more than half of the market makers on the Chicago Board of Options Exchange, now utilize valuation models developed by Merton [1973] and Black and Scholes [1973], all academic searchers in the area of finance.

The third product of academic research in finance which is rapidly being adopted by our larger institutional investors is an advanced method for adaptive estimation of future security and portfolio risk [Rosenberg and Marathe, 1975]. It is based on the most powerful econometric methodology currently available and on an approach which in effect combines technical and fundamental analysis. It is surely one of the ironies of our time that almost no one in accounting is familiar with the underlying research, which reveals that ordinary accounting numbers possess an overwhelming (and reassuring) power to improve on estimates of future risk derived from past prices alone. Why, one must ask, wasn't the research itself done by a scholar in

accounting? This question appears particularly pertinent in view of the fact that the seminal study in this area, analyzed on p. 20 of the Statement, was published in the accounting literature [Beaver, Kettler, and Scholes, 1970].

PARADIGMS IN ACCOUNTING

The 1977 AAA Statement did not specifically concern itself with theory acceptance by *practitioners*, and in that sense, the preceding section may be viewed as a partial digression. One purpose of that digression is to provide additional background for the present section.

Kuhn's model (1970) of the behavior of scientists does offer an interesting perspective from which to view recent developments in accounting. While plausible, Kuhn's (descriptive) model has not, however, been systematically verified. For unexplained reasons, it seems to have received more attention among accountants than among other social scientists.

There is a cautious suggestion in the Statement that the three approaches summarized in Chapter 2 might "... each be treated as an alternative paradigm" (p. 43). I have considerable difficulty with this proposition.

If there is a paradigm in accounting, it would seem to me to have to be closely related with the structure of modern corporate accounting: a focus on assets, on claims to these assets, and on periodic changes in both, with each dimension associated with a *unique standardized number*. (This description is obviously similar (but not identical) to the "true income" approach described in the Statement.) One might argue that this paradigm took hold in the last century and reached its zenith in the 1930s, 40s, and 50s.

By the 1960s especially, considerable disenchantment had set in. The State-

ment attributes this to "... dissatisfaction with the prevailing matching-attaching paradigm" (p. 43). I am more inclined to interpret the problem more broadly, as caused primarily by the shortcomings of relying on the single-number (nominal currency point) estimates to which the double-entry system naturally leads us. (This property of the paradigm can also be viewed as directly responsible for "the allocation problem," which is discussed on pp. 32-33.) In trying to sort out the various anomalies, one group began to look to information and decision theory for help, another grew ever more uncomfortable with our utter lack of systematic knowledge of relevant empirical phenomena. Later, and more independently, a third group began to employ welfare-economic tools in searching for answers to certain accounting policy questions. Each of these approaches has had only modest success, and it seems much too early to think of them as alternative paradigms. The "old" one has not come close to being overthrown and may yet be repaired. For one thing, symptoms of narrow-mindedness should not be confused with paradigms. While scientists are not immune to tunnel vision, many recognize a much larger picture than the one they themselves are working on. In addition, the lessons from the failure of recent attempts to create a grand theory of accounting in one monograph have not been lost. For another thing, neither approach has much to show, for the effort expended, on which one might build a cohesive structure or theory.

There is one sentence in this context which I find particularly puzzling in the Statement: "Logic and empirics do not, therefore, provide a sufficient basis for selecting between competing paradigms" (p. 46). Why is a choice necessary, especially between such narrowly defined

“paradigms” as the “decision-usefulness” and “information economics” approaches? Can’t one be perfectly tolerant of, and learn from, both? Might not portfolio (diversification) theory apply here as well? In any case, what besides descriptive knowledge and logic does *anyone* have to work with in making choices?

GOODS AND BADS

Clearly the best part of the statement is the discussion of the “information economics” approach (pp. 21–25). This is also the area in which accountants have been the least active, by far. The main point is well stated; when it comes to information, there are indeed several reasons why a market system, guided by Adam Smith’s invisible hand, does not work as well as one might like so that regulatory intervention (*e.g.*, with respect to accounting disclosure requirements) should be *considered*. But regulation is costly, so that whether it is actually the right remedy is generally unclear without empirical estimates of the impact (pp. 23, 25) and possibly even with such estimates. In this context, the cost-benefit section on p.37 is too negatively stated. The fact that a complete social ordering of (three or more) alternatives satisfying Arrow’s criteria does not, in general, exist does not imply that we should throw up our hands. *Partial* orderings of the outcome distributions of accounting policy alternatives implementable at the social level can typically be obtained via Pareto’s criterion [Hakansson, 1977]. That is, we may be able to assign grades (A, B, C, *etc.*) to categories of alternatives and conclude that the final choice should be drawn from the A category.

A related question, not considered in the Statement, is the welfare impact resulting from the licensing of public

accountants. A recent (post-Statement) study suggests that licensed professions, under plausible assumptions, have incentives to generate monopoly rents by setting minimum professional standards on the high side [Leland, 1977].

I have no particular praise or criticism of the section on the “classical approach” to accounting theory (pp. 5–10) although I do not share the committee’s confidence in the classification scheme used.

The section on decision models (pp. 10–17) is relatively lengthy and reflects accurately the lack of rigor in conceptualization and reasoning that characterizes so much of the work in the area.

The discussion of behavioral accounting research (pp. 17–19) is the second best in the Statement. The analysis of aggregate market-level research (pp. 19–21) is on the brief side and somewhat incomplete in its coverage; it profitably could have included reference to the interesting study by Collins [1975], for example.

The so-called “data expansion approach” to external reporting is, somewhat surprisingly, examined on pp. 37–39 rather than in Chapter 2. To claim that the “proponents of this approach argue that *more information is assuredly preferable to less*” (p. 37, emphasis in original) is surely an injustice. The cited paper by Sorter [1969] raises a simple but worthwhile point, namely that raw unaggregated data may well be better—within limits—than various aggregate summary measures (such as those provided by the current accounting model), the communication of which generally involves a clear loss of information.

THE STING

After reading the first four chapters of the 1977 statement (on which the preceding criticism is based), I confess that I was not quite ready for the final, summarizing chapter. Not that I hadn’t been

puzzled by the periodic allusion to "theory closure" (pp. 1 (twice), 25 (twice), 41, 47, and 49). But when I read "Our message is clear; *theory closure cannot be dictated*" (p. 49), I felt a definite letdown. Is that really what the Statement is about? Is that as far as an elite committee of the intellectual branch of accounting has managed to bring us in the year 1977? The committee apparently believes that the rest of us equate theory closure (which I presume means a tendency for all concerned, in an environment of free inquiry, to agree on *a* theory) with some kind of utopia. The evidence, please. Furthermore, while utopia may be worth a visit, whoever said utopia was an interesting place to live?

The "second basic message" is that "external reporting theory has a wider scope than that which has been generally perceived" (p. 49): Perhaps this is news for those who were asleep when the ecology wave struck or who missed its point.

The "third basic message is that all theory approaches are flawed when viewed from the perspective of some alternative approach" (p. 50). This statement can be made both briefer and stronger. *All* models in the empirical sciences, being abstractions, or representations, or simplifications of some reality under study, necessarily cannot capture everything about that reality without themselves being complete reproductions of that reality. So the statement holds not only relatively but absolutely and trivially: All models are flawed.

The final "message... is that until consensus paradigm acceptance occurs, the utility of accounting theories in aiding policy decisions is partial" (p. 51). This statement errs by being too generous. As noted in the third section of this critique, the descriptive knowledge and

the prescriptions provided by accounting research only have the *potential* of making policy makers aware of things they did not know before, even when a paradigm is in full bloom. It's what we genuinely learn from our research that counts.

EPILOGUE

As I indicated in the second section of this review, there are many reasons why I am far from convinced that the committee approach makes sense even in summarizing the state-of-the-art. There are few areas in which decentralized production decisions generated by self-interest can be surpassed, and scholarly activities is most likely not one of them. And when most of the members of a committee keep citing their own minor works, the effect is somehow more pronounced than when single or multiple authors do so.

In sum, the 1977 Statement, including the non-italicized part of Chapter 5, is clearly much better than the four (italicized) central messages discussed in the previous section. It does bring a number of seemingly disparate threads together in a way which is, on balance, helpful. It reflects faithfully the recent broadening of the accounting horizon and the gradual lifting of scholarly standards that is currently in motion. It suggests, if only indirectly, that careful attention to less ambitious slices of the accounting problem is essential to further progress. But the accomplishments to date in accounting are on the whole overstated: recent developments in theory, frankly, have not moved us very far forward in the last 35 years in comparison with, say, recent developments in an area like finance. I have suggested elsewhere that advances in finance, economics, and behavioral science are in the nature of a pre-condition for substantial further

progress in accounting [Hakansson, 1978b]. These developments are well on their way, and from this angle the next

10 years or so look exceedingly promising for accounting research.

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